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Criswick's mean as the standard. The following is the result:—

	1877. ″	1878. ″	Mean 1877–78. ″
Lynn —Criswick =	+0·54	...	+0·54
Downing — " =	−1·02	−0·27	−0·72
Thackeray— " =	−1·02	+0·95	−0·10

These numbers seem to show that we must look to some other cause than that assigned by Mr. Neison to explain any increase in the mean tabular error of the Moon. The figures point towards a decrease in the error rather than an increase; but they also prove that it is unsafe to base any particular theory on results liable to such fluctuations as those exhibited in this table.

Kidbrooke, Blackheath,
1880, *January* 8.

Note on the Remeasurement of a Lunar Photograph, in reply to Mr. Neison's Criticism.

By Prof. C. Pritchard.

In the *Monthly Notices* of November last, Mr. Neison did me the honour of criticising a paper of mine laid before the Astronomical Society in June of the same year. In that paper I presented to the Society what seemed to me to be a question of considerable novelty and interest, viz. the measurement of the Moon's mean semi-diameter from a series of photographs taken by Mr. Jenkins with Mr. De La Rue's Reflector, the admirable defining power of which is generally acknowledged. I explained that the computation ef the Moon's diameter was not the ultimate object for which the photographs were originally taken, but that, if I found a considerable number of them, taken at random out of many hundreds, gave accordant results, this circumstance would give me confidence in applying the fine series of photographs in the possession of the University of Oxford to the determination of the lunar physical libration; the latter investigation, it may be observed, involving an enormous expenditure of labour, and not to be undertaken without adequate grounds of confidence. The results of the computation of this diameter exhibited an amount of accordance which not only satisfied, but surprised me, when I reflected on the discordances arising from other known methods of observing. These results of very laborious measurement and computation I laid before the Society, and I thought them creditable both to the photography and to the computation.

Mr. Neison, however, in his criticism thinks it right to say: "Until we know the method by which these measures have been reduced, it is impossible to say how far the above result indicates

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the true photographic semi-diameter, or how far it may be vitiated by the effect of the inequalities on the limb. In the method employed by Wichmann, the effect of these inequalities is not eliminated; and if Professor Pritchard has employed the same method, they will not have been eliminated from the above result."

The method of measurement was stated by me in the second and third paragraphs of my communication of June last. Substantially it is Bessel's with some modifications suggested by the peculiarity of Mr. De La Rue's measuring engine. As to the mode of reduction of these measurements, this also is substantially Bessel's; and in fact is the only natural one that would suggest itself to any mathematical astronomer.

However, I hope before a few months are over, to lay the matter in all its details before astronomers, so soon as the second fasiculus of the work done in the last two years at the University Observatory of Oxford can be published. Still, as Mr. Neison's criticism seemed to me to have a tendency to throw some doubt over the substantial accuracy of the method having in view the inequalities of the lunar contour, I arranged for the remeasurement of the semi-diameter of some one photograph taken entirely at random, and that by both the Observatory Assistants, superadded to a measure of my own.

The photograph selected was taken on January 26, 1877, just three years ago. Mr. Plummer selected, at his own judgment, eight points on the limb, measuring their distances from the centre of *Hypatia* B. Mr. Jenkins selected seven other points on the same limb, and measured from the same spot. Measures taken by myself, Mr. Plummer, and Mr. Jenkins of the same distances were compared together and the means of our measures did not differ by more than $0''\cdot15$ in about $600''$. Consequently no appreciable "personality" existed in the results of our mode of measuring these lunar distances. Neither can I see how such "personality" could exist in a microscope measure, when due care is taken to destroy parallax and to ensure all proper and obvious instrumental adjustments. The measures were then subjected to computations, the same as would be adopted by any competent astronomer.

Now let it be admitted that there are irregularities in the lunar contour, as assuredly there are; nevertheless here are measures, taken from a well-defined point on the lunar photograph to 23 points on the limb, in parts of it of more than average evenness; and what is the result? The original photographic mean semi-diameter was, as is stated (page 449, line 14) $15' \, 34''\cdot26$. The new measures of January 1880 give

	′	″
For Mr. Plummer	15	34·58
„ Mr. Jenkins	15	34·02
The mean is	15	34·30

which shows, I think, conclusively how very little the first result is affected by the introduction of 15 fresh measures. Further still, I would appeal to astronomers who are experienced in delicate and accurate micrometrical measurement whether a greater degree of accordance could be expected even if the Moon's contour were absolutely uninterrupted. I trust Mr. Neison may not think it desirable here to repeat his former criticism of "fortuitous coincidence." I claim for them the natural results of some instrumental experience and some astronomical skill.

Oxford, Jan. 9, 1880.

The Lithograph of the *Pleiades*, including the Nebula round *Merope*, belongs to Mr. M. Hall's paper, "The Nebula in the *Pleiades*," printed in the December No., pp. 89–90.

Changes of Relative Brightness of Jupiter's Satellites.

By C. E. Burton, Esq.

Perhaps I may be permitted to call the attention of those Fellows of the Royal Astronomical Society who are interested in the changes referred to in the above title to an essay by Dr. Rudolph Engelmann, of Leipzig, *Ueber die Helligkeitsverhältnisse der Jupiter's Trabanten*, Leipzig, 1871, which treats very fully of the connection between the brightness of the satellites and their places in their orbits, establishing the reality of such a connection in the case of IV., even going so far as to identify the period of axial with that of orbital revolution with certainty for this satellite, and rendering it probable that the first satellite also behaves similarly.

Perhaps I shall be pardoned for calling attention to the concluding paragraph of a Note on IV. published by me in the *Monthly Notices*, vol. xxxiii. pp. 472–4, years before I heard of Engelmann's results or labours, and taking a somewhat different line of argument from his, which leads, nevertheless, to the same result.